

PAA 100
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[D. H. Campbell]
[from almost illegible thermofax, July 9, 1998]
[retrieve from /fs1/jsl/TX/20.campbell]

Dear Dr. Campbell:

Thank you for your reprints which arrived just before my departure.

I hope my remarks about your "scepticism" were taken as lightly as they were intended -- perhaps they reflect the very numerous scepticisms I have already received from many immunologists with whom I have discussed elective theories of antibody formation. I must confess that my own first reactions to Burnett's proposals (particularly in their original form, stressing clonal populations fixed at birth) were equally skeptical. But after having failed to find compelling arguments against it, and having patched up a few minor implausibilities, I feel the theory ought no longer to be overlooked, and deserves to be attacked on the events of its propositions. I would like to provoke such attack to find out its weakest points! (and possibly still patch these up). *

In this picture, and especially in the model for "tolerance", the persistence of antigenic fragments plays a central role. In fact I have put off some while (and Burnet may still be misled) by Medawar's deflections of this issue, e.g. in his distinctions of tolerance vs paralysis as the grounds that antigen may not have persisted in the former!

I am looking forward to seeing you on the coast, and I know Gus Nossal shares this -- he is coming to Stanford for a year or two in July. I'll be there in February and March, then away for 3 months before properly settling down.

*You should be receiving a better amplified statement of a selective theory when the ms. is mimeographed at Madison. Your critical comments will be quite welcome, though it may be some time before I could answer them.

Could I also ask you for reprints of your other more immuno- chemical papers? Especially those on specific adsorbent technique, which are most provocative. Please send these direct to Genetics Dept (B), Stanford.

Yours cordially,

Joshua Lederberg

(B) stands for mail not to be forwarded

I probably won't be travelling much before next summer. If you find yourself in the vicinity of Stanford in Feb or March to pay us a visit.

J.L.

PPS White has a paper in mid-November Nature also supporting monospecificity of ABF cells. The ms. being sent discusses Lennox and Cohn's very interesting finding of bi specific cells after serial immunization.

We (Bill Stone etc.) have some expts. midway with bovine RBC to clarify the question of simultaneous entry of 2 antigens. The same with mosaic-antigenic flagellins. We should, I know, be thinking more of synthetic haptens.

PS III If you have a duplicating machine could you send back a copy of this note? Don't take any trouble over it.